

strenuous race of men. The author's particular text is the function of the State in fostering agriculture; left to himself, the farmer is normally a strong individualist, who readily becomes isolated and hide-bound. His sole chance of success in modern life is collective action, and Prof. Bailey discusses in successive chapters the extent to which the State can profitably intervene in the organisation of rural life by education and by starting various forms of cooperative work which will lead the farming community to act together. Different as the agricultural conditions are in this country and in America, the problems are the same in both places, and Prof. Bailey's discussion of the subject gains a special interest for us at this moment, when the Chancellor of the Exchequer has just set aside a "development grant" to be devoted to the promotion of all agencies for encouraging rural life.

The Problem of the Feeble-minded. An Abstract of the Report of the Royal Commission on the Care and Control of the Feeble-minded. With an introduction by Sir Edward Fry, G.C.B. Pp. x+113. (London : P. S. King and Son, 1909.) Price 1s. net.

THE appearance of this abstract is most opportune. The small committee of persons interested in social problems which is responsible for its publication is anxious to bring before as large a section of the public as possible the urgency of the matters in question, and points out in the preface of the book that the Poor Law Commissioners have given it as their opinion that if the recommendations of the Commission on the Care of the Feeble-minded were carried into effect, a system of control over the feeble-minded would be initiated which would free the Poor Law administration from one of its greatest difficulties and, we may add, the country from a cause of enormous expenditure. In his introduction Sir Edward Fry quotes Bagehot's sad reflections upon the undue haste and benevolent thoughtlessness with which so much philanthropic effort is attended, and the terrible question which he poses as to whether the benevolence of mankind does not do more harm than good. Sir Edward Fry can, however, recommend the work of the Commission on the Feeble-minded as one done with deliberation and not with "a wild passion for instant action." The various problems which came before the Commission, such as mental defect and drink, mental defect and crime, and mental defect and illegitimacy, are adequately epitomised, and the far-reaching recommendations of the Commission duly considered as to the essential points. The book also contains some special articles, of which that upon segregation, by Mr. Galton, we can especially recommend to our readers.

The Economy and Training of Memory. By Henry J. Watt. Pp. viii+128. (London : Edward Arnold, 1909.) Price 1s. 6d. net.

THE training of the memory is undoubtedly a part of any good education, and it has hitherto been too much the peculiar field of the faddist and of ingenious but ignorant *a priori* system-makers. This little book, which aims at making of practical value to student and teacher the results of scientific experiment into the subject, is therefore to be welcomed. It is true that some of its precepts appear obvious, but where there are so many conflicting truisms the selection of the right obvious is not unimportant; and much definite information is given on particular points where the merely empirical adviser is quite at a loss, e.g. the advantages and disadvantages of specific types of mental imagery, and the variations of method corresponding to differences in the material to be memor-

ised. Moreover, if the book did no more than free the ordinary adult from that excessive distrust of his memory, which is so bad in effect, and is, perhaps, too optimistically believed by Mr. Watt to be quite unfounded in fact, it would be abundantly justified.

Mr. Watt considers the mechanical memory of association to be, in a sense, more fundamental than the intelligent memory based on connection of thought, since the association between word and "meaning" is in itself mechanical. It seems doubtful if that ultimate "association" of meaning and imagery can be expressed so simply; but the point, though of great interest, is of minor importance in a confessedly practical book.

LETTERS TO THE EDITOR.

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Rate of Helium Production from the Complete Series of Uranium Products.

A KNOWLEDGE of this constant is essential to the estimation of the ages of minerals from their helium content. In a paper published in Proc. Roy. Soc., July 28, 1908, I gave the ages of some minerals provisionally on the assumption that the rate was 9.13×10^{-8} c.c. per gram U_3O_8 per annum. This rate was calculated from Rutherford's indirect data. It has received much support from Sir J. Dewar's determination of the rate of production by radium with its immediate products. I am now in a position to confirm it further by an experiment on the rate of growth of helium in a solution of pitchblende; I speak of a solution, but it has been found impracticable to take up all the constituents by one solvent. Two solutions were necessary.

The pitchblende solutions contained 115 grams of U_3O_8 , and yielded in sixty-one days a quantity of helium which was measured as 2×10^{-6} c.c. in the capillary of a McLeod gauge. This gives the rate as 10.4×10^{-8} c.c. per gram U_3O_8 per annum. No stress can be laid on the close agreement with Rutherford's estimate in view of the very small gas volume measured. The experiment proves, however, that that estimate is of the right order of magnitude. Larger scale experiments are in progress, and these, in conjunction with similar experiments on thorite, will, it is hoped, enable data on the quantity of helium in minerals to be translated into estimates of time with full confidence.

R. J. STRUTT.

Imperial College of Science, South Kensington,
July 27.

A Kinematic Illusion.

PEOPLE are sometimes amazed by noticing that in a motor-car seen through railings the wheels appear to revolve the wrong way. As the eye follows the moving objects it is convenient to imagine that the car, which may be actually running to the right, is stationary, while a vertical rail is moving past it to the left with an equal velocity. The apparent intersection of this rail with the upper edge of the wheel is a point running round in a contrary direction to that of the rotation of the wheel. This moving point suggests rotation of the wheel. When oblique lines swing in front of vertical lines the movement of the intersections is curious to watch. It is true that the lower half of the wheel goes against our theory, but at a given moment its effect may be less noticeable, either from being hidden in dust or because the eye has a very small range of close attention. I have seen the appearance, and have had reports of it from others, but cannot speak with precision as to the condition of seeing it effectively.

It is common to rotate vacuum tubes while a discontinuous spark illuminates them. A spark may pass at the instant of starting one revolution, and the illumination

may recur slightly before or after the beginning of a second round; in either case there is a false suggestion as to the rotation. The railings would make discontinuous vision of the spokes of the motor wheels, and a spoke might be seen upright in one gap but at slightly different angles at other gaps. I do not feel that the solution of the problem lies in this direction.

Winchester College.

W. B. CROFT.

Natural Selection and Plant Evolution.

MANY readers of NATURE must have been browsing with delight in the goodly volume on "Darwin and Modern Science" which Prof. Seward, of Cambridge, has taken such admirable pains to collect. Of all its many chapters few are more significant than that on the palaeontological plant record by Dr. D. H. Scott, because there, perhaps for the first time, the evidence of the fossils with regard to the influence of natural selection has been fairly tackled by competent hands.

Dr. D. H. Scott does not attempt to maintain that the record to-day is nearly so imperfect as it was when Darwin wrote his famous chapter thereon, fifty years ago. Dr. Scott's namesake and collaborator from Princeton speaks even of the record as, in some parts, "crowded with embarrassing wealth of material"; and yet what about evidence of natural selection? The present writer ventured to say (*Contemp. Rev.*, July, 1902, p. 83):—"Modern palæobotanists furnish us with next to no evidence at all of the work of Natural Selection in evolving new species." Prof. Seward vehemently challenged my statements next month; yet, seven years later, Dr. Scott feels constrained to tell us:—"As regards direct evidence for the derivation of one species from another there has probably been little advance since Darwin wrote."

To put it more plainly, Dr. Scott is forced to admit that he can adduce absolutely no satisfactory evidence at all. All he does is to affirm his own firm conviction (as it is Prof. Seward's too) that natural selection must have been the chief agent; and he instances two cases where he thinks the possible inference extremely plausible, viz. (1) the case of the pollen tube, quite absent in the Palæozoic seed-plants, found very short and imperfect in the living cycads and ginkgos, and fully developed in the angiosperms, but fossil proof of linking forms there seems none; (2) the embryo in the angiospermous seeds, whilst Palæozoic seeds contain none. It may, as he says, be "impossible" to some "to resist the conclusion" that the nursing of the embryo by the seed was a process of adaptation. But, at any rate, there is no fossil proof thereof; and yet, as Dr. Scott will scarcely deny, there surely ought to have been some hint and trace thereof, the record being so comparatively rich and full as it is. In the case of the Tertiary mammals the action of natural selection can be very clearly demonstrated in numberless cases. If natural selection was the factor in plant evolution too, why is the record so obstinately silent?

Dr. Scott, like Prof. Seward, takes refuge in the thought that our plant record, for many purposes, begins far too late. "An immense proportion of the evolutionary history lies behind the lowest fossiliferous rocks." My chief object in writing this letter is to ask, Is there any valid proof of this in regard to *land plants*, the matter specially in hand? Their record begins, actually, in the Upper Silurian, and though it is very, very meagre and imperfect, the traces are too widespread to be denied. To deny the existence of known Upper Silurian plants is rankest scepticism, though Dr. Scott avoids all reference to them whatsoever. Why, even so very cautious an investigator as Mr. Robert Kidston tells us of "a plant showing woody structure," a plant so high as that, in the Lanarkshire Ludlow beds (Summary of Progress of Geol. Survey for 1897, p. 74). The most important Upper Silurian plant-remains are probably those from the Tanne Greywacke of the Harz, a fairly numerous and well-developed series, of age a good deal lower than Wenlock. Drs. Scott and Seward ("Encyclop. Brit.", Supplement) wish to pronounce all these fern-like and other plants Devonian, because of their facies; but Sir Archibald Geikie ("Text-book of Geology," ii., p. 976) tells us that these Tanne

plants are found a long way below shales with graptolites, which surely is proof enough of Silurian age.

We have, then, fair evidence of land plants in the Upper Silurian. Our very first air-breather or land animal, a cockroach, comes from the top of the Lower Silurian; and the fossil record of the whole Silurian is rich, varied, widespread, without gap. Yet it yields no hint of conditions favourable to land life below the top of the Lower Silurian. Is it, then, scientific to postulate dogmatically land plants earlier than the Silurian, simply because a theory requires it? Dr. Scott admits quite freely that the known facts go the other way.

He tells us not only of the opposition of the mighty like Nägeli, he also tells us that, as regards the succession of species, there are no greater living authorities than Grand'Eury and Zeiller, and that, in their opinion, "the evidence from continuous deposits favours a somewhat sudden change from one specific form to another." This is most certainly true of the palæontological record as a whole. The evidence is overwhelming here, if only our men of science would be brave enough to forget their theories for a little while. Why insist on exalting the *a priori* methods of the schoolmen on the fair field of modern science? Why insist on refusing all evidence that does not suit? Why? Surely it is not, and cannot be, to enjoy the pleasure of barring out all design from the world in which we dwell.

JAMES B. JOHNSTON.

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Musical Sands.

I CANNOT call to mind the occasion upon which Dr. Irving suggested that grains of hyaline quartz might produce the notes from musical sands, but, as a matter of fact, the grains do not "ring," or vibrate individually as sonorous bodies, and there is no apparent resonance or sensible continuance of the note after the plunger is withdrawn. I do not think any particular variety of silica is essential, because coral-sand is often musical, and my artificial musical sands are made up almost entirely of silicate of iron.

I have already shown that the natural sorting action of winds and waves is a requisite condition for the formation of musical patches on sea-beaches.

Mr. M. S. Gray's letter in NATURE of July 29 giving interesting particulars of his visit to the musical sand-hill near Copiapo, in Chile, confirms the statements made by the inhabitants to Darwin in 1835. In his "Journal during the Voyage of H.M.S. *Beagle*" Darwin referred to this hill of sand as "El Bramador"—"the roarer or bellower," but he did not personally visit the spot.

The extraordinary sensations experienced by Mr. Gray were probably similar to those which have been described by the various writers who, from time to time, visited Rig-i-Rawán and Jebel Nakous, both of which were referred to at length in my paper on musical sands in 1888. Particulars of the artificial production of notes from certain sands were also given by me in NATURE of August 6, 1891.

CECIL CARUS-WILSON.

A Question of Percentages.

If a student obtains 37 out of 50 in one paper, 50 or full marks in a second, and 71 out of 100 in another, what is his percentage on the three taken together? If we add the marks as they stand we get 158 out of 200, or 79 per cent. If, on the other hand, we double the marks on the first two papers, we have 74 per cent., 100 per cent., and 71 per cent. If we add these we get 245 out of 300, or 81 $\frac{1}{2}$ per cent. Will any of your mathematical readers kindly tell me which of these two different results is the true percentage for the three papers taken together? The answer may be very obvious; I can see that the two results must be different, but I cannot see which is the more correct method to use.

J. T. CUNNINGHAM.

60 Milton Park, Highgate, N., July 24.